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ABSTRACT

This paper presents some inferences about preschool education, based on evaluative research done on the Bereiter-Englemann program. These inferences may be summarized as follows: (1) The Bereiter-Englemann program has clearly had more impact on IQ and achievement than the traditional child-centered approach, but not necessarily more impact than other programs having a strong instructional emphasis; (2) the traditional nursery school and kindergarten program has failed to achieve as good results in cognitive learning as the more instructional approaches, and at the same time has not demonstrated any redeeming advantages; and (3) however encouraging the immediate results and follow-up achievement data of a preschool compensatory education program, no preschool program shows promise of making, by itself, a permanent difference in poor children's scholastic success. It is suggested that no more resources be devoted to the search for better preschool programs because the existing technology enables young children to be taught far more than they can benefit from. It is proposed that educators direct more effort toward constructing articulated programs that permit teaching in preschool what will be useful later, and teaching in the elementary school what builds upon the child's learning in preschool. (NH)

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An Academic Preschool for Disadvantaged Children: Review of Findings

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In this paper I shall set forth some generalizations about preschool education based on evaluative research that has been done on the academic preschool for disadvantaged children, more familiarly known as the Bereiter-Engelmann program. To base general conclusions on what is essentially product-testing research is, of course, a highly speculative if not fanciful undertaking. There is no true separation of variables in product-testing research, only the single variable of product entity, and so it is difficult to defend any generalization beyond the products tested. Nevertheless, I think the effort is worth making for two reasons. The first is that there is not much else besides product testing research to base inferences on when it comes to questions of what leads to what in preschool education. There are the careful studies of Carolyn Stern and Evan Keislar that investigate, for instance, the conditions under which verbalization does or does not facilitate learning in young children; but these studies stand pretty much alone. For the rest, we have either program evaluations that do not separate variables or laboratory experiments that are remote enough from real-life learning that inferences from them are equally speculative.

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Another reason for trying to draw inferences from product-testing research is that unless general inferences can be drawn from it the

research will have been largely wasted, since the products themselves vary from one study to another and tend to be obsolete by the time the testing is completed--certainly by the time that long-term effects have been evaluated. Thus, in the case of the Bereiter-Engelmann program, the target of evaluation studies has never actually been the program as set forth in the 1966 book, Teaching Disadvantaged Children in the Preschool, but rather local modifications or subsequent revisions of it. And the original program has anyway since been superceded by the DISTAR program of Engelmann and others, the Conceptual Skills Program of Bereiter and others and most recently, the Open Court Kindergarten Program of Bereiter and Hughes, all of which differ from each other and from the original program on practically any dimension one might name. Thus even a simple statement of product evaluation requires judgments about what factors in a given program are central and about the extent to which these factors have been controlled.

1. The Bereiter-Engelmann program has clearly had more impact on IQ and achievement than the traditional, child-centered approach, but not necessarily more impact than other programs with a strong instructional emphasis. This conclusion has been borne out consistently by all the studies that have compared, over the term of a school year or more, the Bereiter-Engelmann program with a traditional approach: (Erickson et al, 1969; Karnes et al, 1969; Di Lorenzo et al, 1969; Weikart, 1969; Miller & Dyer, 1970; Bissell, 1970). The results have been replicated widely enough and with a great enough variety of cognitive and achievement measures that there is no serious problem about generalizing the conclusion to diverse populations of disadvantaged children or to diverse measures. The

problem rather is to decide to what kinds of treatments the conclusions can be generalized: what are the limits of the categories, traditional, child-centered approach and programs with a strong instructional emphasis?

I have elsewhere (Bereiter, 1970) discussed the common body of cognitive content that seems to be found in preschool programs of all types: identification of colors, shapes, numbers, letters, materials, parts of objects, uses, actions, and the use of prepositions, comparisons, categories, and logical operators. This content has been dealt with in traditional preschool materials and activities and figures prominently in intelligence tests for young children. By programs with a strong instructional emphasis I mean ones in which the teacher's activities are specifically geared toward seeing to it that every child masters this content. In the traditional or child-centered approach the teacher's activities may be intended to promote learning of this content, but the teacher is not held responsible for seeing that the learning actually occurs.

Among the programs that emphasize instruction there are conspicuous differences in method and less conspicuous ones in content. Some investigators have also tried to distinguish them on the basis of broad goals, but in the absence of observational evidence to the contrary, I regard such distinctions are purely rhetorical. Bissell (1970), for instance, distinguishes a structured-cognitive approach, exemplified by the Ameliorative program of Karnes and a structured-informational program, exemplified by the Bereiter-Engelmann program-- the former emphasizing cognitive processes and the latter supposedly ignoring these and concentrating

upon "correct" responses. I observed these two programs operating side-by-side for two years and, while I could detect many differences in method and organization, I did not observe anything to support the distinction Bissell has made. Such distinctions derive, as best I can tell, entirely from the kinds of theoretical ornamentation that program originators use to raise the tone of their reports.

This is not to say that the instructional programs are effectively identical. In comparison with other instructional programs, the Bereiter-Engelmann program has tended to show higher immediate IQ gains on the Stanford-Binet (Miller & Dyer, 1970; Karnes et al, 1969; Weikart, 1968, 1970). The Karnes program has shown higher gains in reading readiness, the DARCEE program on Peabody Picture Vocabulary. These differences can be plausibly explained by differences in content emphasis. The Bereiter-Engelmann program contains relatively little work on vocabulary development or paper-and-pencil exercises of the kind used in readiness testing, but does entail more verbal reasoning and problem-solving.

Having noted that different instructional programs appear to teach somewhat different things, we need not analyze the differences any further, as if to tease out all the differences and then weigh them up to decide which program offers the most of what. Such comparisons may be worthwhile if one is shopping for a program to install, but they are not instructive.

The reason they are not instructive is that there is very little evidence that learning one thing does more good than learning another. There are some studies that indicate that learning letter sounds does more good than learning letter names as reading readiness training (

There is also a substantial amount of negative evidence on the value of learning the kinds of things taught in perceptual-motor training (

. It would be helpful to have some evidence on the relative value of vocabulary building versus training in the more precise and flexible use of words already known--on the transfer value, that is, of such learning to worthwhile tasks like reading. Merely to know that one kind of teaching yields better scores on a vocabulary test and the other on the Basic Concept Inventory is not much help. It is not known whether gains on these tests, or on general intelligence or readiness tests either, are of any value.

From the data available on transfer of preschool treatment to later school learning, we have no grounds for distinguishing between programs with strong instructional emphasis. Again, they do better than the traditional, child-centered programs (Bissell, 1970), but do not differ noticeably from each other. Comparative data on transfer effects are much scantier than on immediate effects, however. The only strictly comparable follow-up data are those for the Karnes and Bereiter-Engelmann programs (Karnes et al, 1969), where no differences in subsequent achievement appear, although both show achievement in first and second grades superior to that obtained with children in a traditional program.

One seemingly implausible conclusion that may be drawn from the studies to date is that all programs that have set out in a deliberate fashion to teach the core content of preschool education have succeeded, no matter how they have gone about it. The conclusion is probably not true, in that the programs under consideration here are ones that had enough success to have enjoyed continued funding for long enough to carry

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out extended evaluations and to have been investigated by researchers interested in comparative evaluation. On the other hand, I think it is reasonable to say that the core content mentioned earlier, consisting largely of everyday concepts, is not very hard to teach. It is not like phonics or fractions where, if the teacher is not careful, she can muddle the children's minds so that they not only don't learn it but are rendered to some extent incapable of learning it thereafter. From this standpoint it may be said that the differences in method represented in the various instructional programs have not been put to adequate test. They would need to be applied to the teaching of something difficult. Reading and arithmetic have been taught in the Bereiter-Engelmann program These are hard to teach, and they were taught with success: children at the end of kindergarten were averaging second-grade level in word recognition and in arithmetic computation (Bereiter, 1968). Since the other programs have not tried to teach anything this difficult to children so young, there is no evidence to say they couldn't do it. I have only my own experience to go on in saying that I do not think that the more casual, unprogrammed kinds of instruction that characterize programs other than Bereiter-Engelmann are equal to the task of teaching anything difficult.

Special note must be taken of the showing made by Montessori classes. Three of the studies mentioned thus far included Montessori classes among the treatments compared (DiLorenzo, 1969; Karnes, 1969; Miller & Dyer, 1970). In all of these the Montessori classes produced results similar to those of traditional classes and thus inferior to those that I have been calling instructional approaches. The Montessori method is so unusual, of course,

that it is going to make a strange bedfellow no matter what category of program it is put into. Bissell (1970) labels it a "structured-environment" approach and puts it into a category with the "New Nursery School" of Nimnicht and Meier. Such a designation is reasonable, but doesn't take account of the very elaborate and systematic pedagogy of sense training and concept teaching which the Montessori method entails. It is more accurate, I think, to treat the Montessori method as the special case of an instructional approach that hasn't worked very well, at least in its present applications. The problem could be one of lack of transfer from non-verbal training to verbal testing, although in the Miller & Dyer (1970) study, the Montessori group made one of its poorest showings on the Embedded Figures test. My own surmise is that the Montessori method is simply pedagogically defective in that it does not present enough different examples of a concept to permit a child to grasp it--a defect which other approaches are spared either by design or through the good graces of random variation.

2. The "traditional" nursery-school and kindergarten program is not a serious contender as an educational program. Not only has the "traditional" approach failed to achieve as good results in cognitive learning as the more instructional approaches, it has failed to demonstrate any redeeming advantages. In the Kalamazoo and Louisville studies a variety of motivation and adjustment measures were taken: in the Kalamazoo study (Erickson et al, 1969), teacher ratings of adjustment, observer tabulations of deviancies, and records of attendance; in the Louisville study (Miller & Dyer, 1970), ratings by teachers, ratings by testers, and scores on the Cincinatti

Autonomy Battery. By none of these indicators did the traditionally taught children show themselves to be better off than these in the more instructional programs. In the Kalamazoo study they were significantly lower, although superior to controls.

One of the cleanest sets of results is from the Kalamazoo study's analysis of kindergarten attendance records. Here children who had been in a Bereiter-Engelmann preschool showed higher kindergarten attendance than those who had been in a traditional preschool, who in turn showed higher attendance than those who had not been to any preschool. But within each of these three groups, those who attended a Bereiter-Engelmann kindergarten showed higher attendance than those who attended a regular kindergarten. Now it is not at all clear what child characteristics attendance is an indicator of; but the same may be said of any other available measure of childhood personality and adjustment, impressive test labels notwithstanding. One thing that can be said of attendance that can not be said confidently of test variables is that it must indicate something important and not some trivial instrument factor. School attendance would seem to be a social indicator, a very gross index of how well things aregoing with a child in relation to school. Its very lack of specificity guards it from the complaint that can be made against other variables in the evaluation of preschool effects, that they do not do justice to the broad socio-emotional goals of a child-centered program. I do not know any way to interpret a difference in school attendance in favor of children in the B-E program that is not damaging to the claims made for the traditional child-centered program.

Experimenters who have used a "traditional" program as one kind of treatment have all evidenced difficulty in defining what such a program is. The name itself, of course, isn't descriptive of what goes on and is regarded by many early childhood educators as pejorative. Yet even to find a name that distinguishes it from competing programs is difficult. Early childhood educators have also complained to me that there is no such thing as a "traditional" program or a "regular" Head Start approach, that they differ widely. Such differences, however, have always escaped my observation and apparently they also escape detection by systematic classroom observation. (Lois-Ellen Datta, private communication, reports that efforts to study the effects of natural variations among Head Start programs have had little success because there simply was not variation to work with.)

Miller and Dyer, in the Louisville study, offered a systematic point-by-point comparison of the four types of program they studied. The "traditional" approach, interestingly enough, is largely distinguished from the rest on the basis of things that are not done. The video-tape monitoring of teacher behavior in the same study provides striking support for the ideological distinction. Teachers in the "traditional" program are not so much distinguished by differences in the relative frequency of different kinds of teaching acts (as are teachers in the other three programs) but by the generally low frequency of teaching acts of any kind. The mean frequency of teaching acts of any kind among the "traditional" teachers is less than half that of teachers in the Bereiter-Engelmann classes (Miller & Dyer, 1970, p.53).

Furthermore, the only categories of behavior in which "traditional" teachers showed up as noticeably more active than teachers in the other programs are Contingent Negative Verbal Reinforcement, Conduct Modification, and Academic-Verbal Giving (lecturing).

The picture that emerges from these results is one that accords with my own observations. It is that the "traditional" approach does not represent a <u>different</u> way of teaching from those represented in newer programs but simply represents a lower order of program, one that is more custodial and less purposefully educational. The lesser overall amount of teaching behavior and the greater emphasis on behavior management suggest the custodial function. The greater use of straight verbal presentation as a way of giving information entirely out of keeping with "traditional" doctrine if it is taken to indicate deliberate pedagogical method. It is quite understandable, however, on the assumption that instruction occurs only incidentally in "traditional" classrooms, without prior planning, so that the teacher is not prepared to communicate information in any other way than through just talking. To demonstrate or model a concept, to ask leading questions, to develop a concept through sequenced tasks--any of these require more preparation and a more deliberate intent to teach than is found in the "traditional" class.

It seems to me somewhat misleading to go on treating the "traditional" approach as one among a host of alternative approaches to teaching young children. It is better seen, not as a distinctive approach to teaching, but as a system of custodial child care that may incorporate to a greater or lesser extent various educational components similar to those found in

instructional programs for young children, but that is primarily distinguished by its minimization of teaching. The true issue between the traditional approach and the various instructional approaches is not how young children should be taught but whether. This is still a live issue, far from having been settled by research. It is to this issue that I now turn.

3. The long-term effects of preschool instruction are about as good as can be expected. However impressive the immediate results of preschool compensatory instruction may be, and however much encouragement may be drawn from follow-up achievement data, the fact remains that no preschool program shows any promise of making, by itself, any permanent difference in the scholastic success of poor children. Excuses for this fact are so easy to come by that I will not bother to enunciate any. One practical implication is also obvious: that increasing the scholastic success of poor children will require effort across the span of school years and not just a pump-priming operation at the beginning.

Less obvious, however, are the implications for further work on preschool education. Two questions need to be considered:

a. If it is granted that education for poor children must be improved over the whole span of school years, then is it any longer necessary or practical to invest heavily in preschool education for such children? In other words, is preschool education anything more than the stone in the stone soup?

b. Is there justification for heavy investment in a continued search for more effective methods of preschool education, or have the

limits of effectiveness pretty much been reached?

Both of these are policy questions that have to be acted upon whether there is any pertinent evidence or not, and so whatever faintly valid evidence may be dredged out of evaluation studies is that much to the good.

The cross-over data from the Kalamazoo study afford the only evidence that I know of that is directly pertinent to the first question. Put more crudely, the first question reads, if you are going to follow up anyway, does it make any difference what you follow up on? The Kalamazoo study found that children in regular kindergarten classes did better if they had been in a Bereiter-Engelmann preschool than if they had been in a "traditional" one, or had had no preschool at all. On the other hand, children from these three preschool conditions who went into a Bereiter-Engelmann kindergarten all ended up at about the same level of performance. If the Bereiter-Engelmann kindergarten is taken to represent "follow up," that is the continuation of special treatment, then it would appear that it does not make much difference what one follows up on: the preschool treatments could have been eliminated without loss. On the other hand, when there was no "follow up"--that is, when children were put into a regular kindergarten program--performance was highly dependent on the nature of preschool experiences.

This finding is pregnant with implications. Consider, for instance, how the results might have been interpreted if all the children from the three preschool conditions had gone into a Bereiter-Engelmann kindergarten and if, furthermore, this kindergarten program had somehow gotten itself established as normal so that no mention was made of what kind of kindergarten program it was. Then the data would have shown that preschool effects

"washed out" when the children got to kindergarten. One might even have been tempted to blame the kindergarten for washing out those grand effects. Under the actual circumstances, however, with traditional kindergarten classes for comparison, it appears that the washing out of effects was a good thing, since it consisted of bringing those children with the less favorable preschool experience up to the level of those with the more favorable.

Extrapolated, these results would suggest that highly effective program at any level of schooling will overcome the effects of variations in educational experience up to that level. The suggestion is probably not true, of course. If it were we could concentrate all our efforts on making a bang-up success of the last year of schooling and not worry about whether children learned anything in the years preceding. But if it is true that an effective kindergarten program can overcome differences in preschool experience, then it may also be true that an effective first-grade program can overcome differences in kindergarten experience, and so on up to some unknown point where the weight of past experience tips the scales.

The wise strategy for the present, then, would seem to be to look for elementary school programs that are more successful than the present ones at washing out the effects of differences in earlier school experience. This strategy, does not, however, preclude the continued search for more effective methods of preschool education. On this matter we have to ask ourselves what increased effects we would want or have any reason to expect were possible.

Such a question invites visionary responses a la George Leonard (1966). Generalizing from what we have been able to teach in our experimental programs, however, I am inclined toward the more pedestrian position that existing technology already enables us to teach young children far more than they can benefit from. What we need to do is not discover ways to teach them more but rather construct articulated educational programs that permit us to teach in the preschool what will be of use later and to teach later what builds upon what was learned in the preschool.

Thus I do not believe we need to be devoting resources to developing a better preschool program because we are in no position to say what a preschool program ought to accomplish that present ones do not. As we noted previously, the various effective instructional programs do not accomplish precisely the same things, but there is no basis for saying that what one accomplishes is more valuable than what another one does. I think, therefore, that we are at a point where development of preschool programs if it is to proceed any farther, has to be joined to elementary school curriculum design. The two questions, "What does a child need to know in order to be ready for first grade?" and "What does a child need to know in order to get the most out of being four years old?" have about yielded their all. The first has yielded the core content of preschool education mentioned previously and the second has yielded such things as handling a paint brush and putting on a coat (to mention only objectives that can be acted on; the second question also gives rise to an abundance of fine sentiments). Only by joining preschool education with elementary school curriculum can we begin plausibly asking the potentially much more productive question, "What

things can we teach a child of four and five that can then be built upon in the first grade and after?"

My attitude toward the failure of preschool programs to produce lasting gains is perhaps cavalier. I realize that the more accepted behavior, which I have on occasions engaged in myself, is to express sincere regrets that things haven't turned out better and then offer up an explanation which, while vague and speculative, makes it clear that I am not at fault. It is also possible to find cause for optimism in follow-up results. Verbal reports from Karnes and Erickson indicate that Bereiter-Engelmann children continue to show achievement advantages over control and traditionally-taught children, as far as the third grade. Weikart children from his original experimental treatment showing achievement advantages as late as sixth grade. To me, however, the most parsimonious hypothesis to account for these persisting advantages is that there was a degree of continuing differential treatment given to experimental group children--by virtue of their being assigned to different streams on entry into regular school. I know this to have been the case in the Illinois study, where the schools used IQ and other scores from the research testing to place children in first grade streams. Differential treatment in the Ypsilanti study may have been even more marked, judging from Weikart's verbal report of a substantially larger proportion of control group children being assigned to special classes for the mentally retarded.

The data on long-term effects of preschool intervention are disillusioning but not, to me at least, discouraging. The illusion that

they serve to dispel is that there is some magic in the early years of intellectual development such that a little difference there will make a lot of difference later. What we seem to be finding instead is that a lot of difference there may just possibly make a little difference later. Weakening of the "magic years" illusion will, I hope, render more credible the position that Engelmann and I have argued from the beginning, that learning in young children is just learning: some things can be taught to young children and some cannot; some of the things that can be taught will prove useful later and some will not; what will prove useful later is not determined by some innate chain of development but by the actual course of real-life events. The corollary that I have argued in this section is that one way to make preschool learning more useful is to alter the actual course of subsequent school events so as to make use of it.